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third series, capillary depression was perceptibly though very slightly unequal, in direction to make readings too high."

In the above quotation 'M' means millionths of atmospheric pressure. The calculated probable error of the thirty readings taken together, is only ninety-two hundredths of a unit in the third decimal place; that is to say, less than a thousandth part of a millionth of atmospheric pressure. The probable error of the three mean results, considered as single readings, is only eleven hundredths of a unit in the third decimal place of millionths. The net result may be expressed as follows, in terms of atmospheric pressure: Considered as thirty measurements:

$$0.000\ 000\ 434\ 60 \pm 0.000\ 000\ 000\ 92.$$

Considered as three measurements:

$$0.000\ 000\ 434\ 60 \pm 0.000\ 000\ 000\ 11.$$

Here we have the measurement of a total quantity of less than half a millionth of atmospheric pressure, with a probable error of only about a fifth of one per cent. of the quantity measured.

To show how small is the effect of variable capillary depression in the large mercury columns, the following measurements were made July 25, 1897. No correction was made of accidental capillary differences, but the columns were always observed with a falling meniscus. The zero of the micrometer was freshly adjusted for each reading, and before each of the six sets of readings the mercury was lowered and then readjusted to the proper height in the gauge head.

	M.	M.	M.	M.	M.	M.
	2.210	2.203	2.209	2.198	2.198	2.202
	.204	.195	.202	.203	.204	.198
	.209	.198	.204	.208	.200	.196
	.203	.204	.210	.200	.196	.208
	.203	.192	.202	.198	.196	.203
Means	2.2058	2.1984	2.2054	2.2014	2.1988	2.2014

Calculating the probable errors we have:

Six mean readings.....2.20187 M. ± 0.00073 M.

All readings.....2.20187 " ± 0.00059 "

The effect of not equalizing the capillary depression is very apparent when these results are compared with the earlier ones quoted. But on account of increased skillfulness of observation, due to long experience, the individual readings of each set are more uniform than before; so that the net result is better.

In this example, we have the measurement of about two millionths of atmospheric pressure, with a probable error of only one part in three thousand, of the quantity measured.

From the foregoing, we may safely conclude that with the apparatus described, small gaseous pressures may be easily measured, with a probable error of less than a thousandth part of a millionth of atmospheric pressure.

CHARLES F. BRUSH.

CLEVELAND, O.

SOME THOUGHTS CONCERNING THE TEACHING OF CHEMISTRY.

IN the preface to a short set of 'Notes Upon Qualitative Analysis,' recently published, I made use of the expression: "There is small doubt that, were it not for the expense of printing, every teacher of chemistry would use a text-book made by himself with either pen or scissors."

In a review of the little book which afterwards appeared in one of the foreign journals, the critic referred to the above sentence, with the added remark: 'Sad, indeed, if true!' He who wrote the criticism is a distinguished chemist, for otherwise his opinions could not find place in so eminent a journal; but the thought crosses me: Is he a teacher? There is a tremendous difference between the specialist who never enters the class-room and the trained instructor who but rarely leaves it.

A man may rank in the highest grade as a scientist, and yet be nothing of a teacher; he may be skillful to the last degree in map-

ping out a line of inquiry tending towards the solution of one of nature's mysteries, and yet be a mere tyro in the art of imparting his knowledge to a class of students. It was the writer's fortune, when a student, to have for an instructor a man of world-wide reputation; but, great as the man was as an investigator, he was a very indifferent 'professor.' It has been my privilege from time to time to attend lectures given to undergraduates by men who, although not professed instructors, stand, nevertheless, at the very forefront of their respective professions; and it has greatly interested me to note how different their mode of presentation commonly is from that followed by men more in the habit of meeting an audience of such a character and more familiar with its peculiarities and methods of thought.

It is trite to say that teaching is a distinct specialty, and that to teach well is the gift of comparatively few; but the fact remains pertinent, notwithstanding its triteness, and is worthy of consideration.

To return to the quotation, I would say that it was written in the light of over twenty-two years' class-room experience, and with what I believe to be a pretty full knowledge of the wants of the average student.

I cannot by any means agree with the critical comment: 'Sad, indeed, if true!' It is unquestionably true that every teacher would find his work more easy of accomplishment could he use a text-book of his own arrangement; nor is there any element of sadness connected with this fact. The composition of classes and the arrangement of courses cannot fail greatly to modify the treatment of the same subject, as presented at different institutions; and it would be small praise, indeed, for the instructor were it said of him that he stuck to his text-book literally, even though such book were of unsurpassed excellence. It is

the class-room enlargement of, or variation from, the text that is of real value to the student, for the points thereby brought out are the ones which he cannot readily secure by private study. At a New York preparatory school, where the methods of instruction were as peculiar as they were excellent, the writer remembers that the few text-books permitted were mostly selected because of their poor qualities, in order that criticism thereof might make a deeper impression upon the class. Of course, it would be easy to carry such a system too far, especially when dealing with advanced subjects; but if the instructor be worthy of his position he cannot, and should not, be entirely satisfied with the matter exactly as it is presented in the best text-book ever written. He should have his own way of presenting his subject, or else he will fail to hold his classes.

"An ill-favored thing, sir, but mine own,"

is a motto that might fit the method of many a successful teacher of chemistry, for there are but few sciences whose elementary teaching calls for so much good judgment in placing the subject-matter in a form easily grasped by the beginner and in selecting illustrations from sources that are both homely and apt. When I said that, were it not for the expense of printing, a man would prefer his classes to use a book of his own making, I wish to be understood as holding that, in order to have such a book of the highest order of usefulness, it must be written for his classes, and his alone. For it is a fact that a man writes for his private use a very different and usually a much more effective book than the one he dares to offer the public. It would appear that books are largely written to please the critics; and if they be so constructed as to pass the ordeal of 'review,' it is entirely a secondary matter whether or not the student is able to readily

grasp their meaning when starting from his point of view.

It being, of course, admitted that chemistry should be so taught as to have its principles firmly retained by the student, the instructor should endeavor to place himself in the student's position and strive to see things from his standpoint. It is immaterial how scientific the arrangement of the course may be if such arrangement does not follow the mental drift of the average learner and appeal to his sense of general fitness. It is for that reason that I cannot sympathize with a separation of the oxides of arsenic by an interval of seventy-five pages from the other compounds of the same element, as is done in one of our best text-books. Such separation may suit the views of the distinguished author and his brother chemists, but the book is not written for them; it is intended for the use of beginners, and beginners do not look at the subject in that apparently disjointed way.

Another difficulty with many of our text-books is that they are much too full during the early portions of the course. They deal with expansions of, and exceptions to, topics at a period when the topics themselves are fraught with entirely new ideas to the student.

Take, for instance, the question of 'valency.' If my experience goes for anything, it is better to allow the beginner to conceive valency as a definite constant for each element, and then at a later stage, after considerable experience with things chemical has been acquired, the subject may be brought up again and more extensively discussed.

Again, let us suppose that the student is at work upon the subject of 'Phosphorus.' Almost the first fact he learns is that phosphorus is attacked by oxygen with exceeding readiness, and that an oxide of the element results.

Is it wise, therefore, to insert in the text that 'phosphorus is incapable of uniting with oxygen if the gas be perfectly pure and free from aqueous vapor?' Would it not be better to allow the beginner to become as familiar as possible with the chemistry of ordinary conditions before venturing into those dimly lighted regions where 'chemical purity,' 'perfect dryness,' 'exceeding heat' or 'exceeding cold' are the disturbing factors?

The student tends to hold the instructor responsible for all irregularities in the science, and, as a beginner, he resents ambiguity. Exceptions and amendatory comments both confuse and discourage him. The time comes later on when to note the peculiar character of this substance, or the exceptional behavior of that, may be of real interest to him; but the establishment of such an interest is a matter of slow development, and care should be taken during the early stages of instruction that great masses of heterogeneous facts be not so piled together as to cause no growth at all.

W. P. MASON.

RENSSELAER POLYTECHNIC INSTITUTE,
TROY, N. Y., May, 1898.

*PROFESSOR SCHENCK'S RESEARCHES ON THE
PREDETERMINATION OF SEX.**

IN view of the fact that Professor Schenck's conclusions as to the power of artificially determining the sex of offspring have served as a nine-days' wonder to some of the lay papers, it seems advisable to lay before our readers a plain statement of his argument, taken without comment from the pamphlet which he has just published.† It opens with the statement that it is impossible to command natural processes, but possible by scientific means to exercise a

* From *The British Medical Journal*.

† *Einfluss auf das Geschlechtsverhältnis*. Von Dr. Leopold Schenck, Professor an der k.k. Universität und Vorstand des Institutes für Embryologie in Wien. Magdeburg: Schallehn and Wollbrück. 1898.